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Interactive comment on "Destabilisation of an Arctic ice cap triggered by a hydro-thermodynamic feedback to summer-melt" *by* T. Dunse et al.

Anonymous Referee #1

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General comments: This manuscript presents a nice and detailed set of five GPS time series and synthetic aperture radar velocity maps from the lower half of one of the basins draining the Austfonna ice cap, Svalbard. The calving flux is also calculated. A PDD record is derived from air temperatures from a distant AWS. A substantial speed-up corresponds to the onset of the melt season, and generally a change towards higher velocities is seen when compared to velocities from the 1990s. The authors explain this by a hydro-thermal feedback. GPR measurements show crevasse formation, partly prior to the period covered by the velocity data. The figures are generally clear and informative.

In the current presentations it is difficult to extract which parts of the observed behavior and observations are due to surge dynamics and which are not. The data has a poten-

tial to contribute to a better understanding of glacier dynamics on Svalbard. Some of the GPS data have been published previously, but it is not clear how the application in this manuscript substantially expands the previous results. While I appreciate the attempt to expand our knowledge about glacier dynamics and the response to a changing climate I do not find the hypothesis sufficiently supported and the discussion does not justify the conclusions drawn. The authors generally should compare and discuss their results in greater depth against existing literature on Svalbard glacier surges, in order to distinguish the surge dynamic component from the suggested hydro-thermodynamic feedback. I simply found it difficult to evaluate the feedback until the surge component if further addressed. I still think, however, that this data has a potential to increase our knowledge of glacier dynamics especially on Svalbard, but the surge aspect needs to be clearly separated from the suggested hydro-thermodynamic feedback in order to show its significance. I therefore recommend for a major revision which addresses the remarks below, and then possibly the contribution of the suggested feedback will appear clearer and the possible consequences will be easier to evaluate.

I found the manuscript a bit fragmented and suggest it to be re-organized and aim and objectives of the study to be better defined in the introduction. The manuscript appears a bit fragmented and could do with a better link between the line of thoughts in the introduction and the discussion. Parts of the introduction are not picked up in the discussion. Parts of the results section belongs in the discussion and vice versa.

A major concern is that the authors state there is an ongoing surge, yet there is no attempt to distinguish the surge dynamics and its effects from the proposed hydro-thermodynamic feedback. The dynamics of Basin-3 first need to be put in a proper context, before considering the relevance of extending the suggested mechanism to non-surging ice sheets with other properties. While surges arise from an internal imbalance, also involving an excess of mass in the reservoir area, external factors may have a different outcome once a surge is in progress, than on glaciers purely subjected to increased melt water input. The study only focuses on surface melt-driven processes

as an explanation to the behavior. It would nevertheless be appropriate to refer some basic literature on the (surface) meltwater influence on dynamics.

The data used in this study have a high temporal resolution for the last six years, however they cover only about half way of the full length of the glacier basin. This should be taken into account. The work of Solheim (1991) shows a good match between the estimated surge cycle period for Basin-3 and the current surge. This could have been mentioned. The authors also show that crevasses were formed in the upper part some years before the detailed study of the changes downglacier. Adding the fact that Svalbard surges are known to be long lasting (cf. Dowdeswell et al., 1991 and Sund et al., 2009), an investigating the dynamics at the higher elevations would be appropriate in order to distinguish between the dynamics possibly resulting from processes other than increases in surface melt water. How does the successive destabilization differ from or resemble the surge development described in previous studies (cf. Murray et al., 2000; 2003 and more recent Sun et al. 2014)? Are there any possibilities of supplying with data between 1990 and 2008?

The authors show that the lower parts of a basin of Austfonna experiences multiannual velocity accelerations, but this finding is not particularly surprising given that the basin is also found to be surging. Multiannual velocity accelerations are consistent with previous studies of other surging glaciers in Svalbard (cf. Murray et al., 2003, Sund et al., 2014, and in other areas Burgess et al. 2012). Taking these into account might help to better distinguish the surge contribution of the dynamics from the suggested hydro-thermal feedback.

Phillips et al. (2013) suggested CHW might facilitate temperature change in the Greenland Ice Sheet due to upward migration of the snow zones, is this case at Basin-3 as well? P2690 L13 states: Over 2002–2008, the climatic mass balance of Austfonna was close to zero (Moholdt et al., 2010a). In addition the fact that Solheim's surge cycle period estimate among other based on total net accumulation matched well, does not seem to point towards a substantial upglacier shift of the ELA.

Furthermore, if there is a rise of ELA at basin-3 causing CHW, this would possibly also occur in the other basins as well? It would be nice if the study reflected on why some areas a more affected than others by the proposed feedback. A previous paper covering an additional basin of Austfonna was partly using the same seasonal speed-up data. Why not demonstrate the suggested mechanism on both basins here as well? This possibly makes it easier to extract the non-surge affected contribution to the process. I found the title a bit misleading since it appears there were changes to ice stream behavior prior to the suggested hydro-thermodynamic feedback; hence the basin was already "triggered". Also the term "destabilization" is a bit vague and strange as the authors state there is a surge, which is defined to be short term and with cyclic reoccurrence and accordingly the fast flow is expected "to slow down or come to a halt within a few years". Finally, what is treated here is only a part of the ice cap.

Figures

"a" and "b" and so on, could be indicated on each figure, not just in the caption. Fig.1b. The outline of Basin-3 could be made slightly more visible. The fonts of the current figures could be possibly be slightly enlarged for better readability, but this depends on the final size of the figures.

Specific comments

P2686 L 5. "Basin-3" or "parts of the" could be inserted before "Austfonna ice cap" as the data does not cover the entire ice cap.

P2686 L9. I'm a bit confused over this sentence, "By autumn 2012, successive destabilization of the marine terminus escalated in a surge", and I'm not sure if "escalated" is the right word here, considering the long surge development in Svalbard.

P2688 L25. "We propose that cryo-hydrological warming may have a drastic effect on glacier dynamics. . ." Please consider using another wording than "drastic". This is used several times.

P2689 L3. For surge duration it would be more adequate to reference estimates for Svalbard which are years to more than a decade, rather than months, since the surges in Svalbard (cf. Dowdeswell et al., 1991).

P2689 L7. Strictly speaking I think Hamilton and Dowdeswell, 1991 suggested a deforming bed surge mechanism for Svalbard, while Murray et al., 2000 added the thermal aspect for Svalbard.

P2689 L12. MacAyeal, 1993 is not the proper reference here.

P2689 L13 “provoke” change to “promote“.

P2689 L14-17. Please add reference.

P2690 L2. ice thickness of up to 600m – referred to Lefauconnier and Hagen, 1991, is this right reference? By the way, they suggested that the previous surge might have been larger than the Brasvellbreen surge in 1937-38.

P2690 L16. It would be useful to get an indication of the approximate length of the basin, especially as the locations of GPS’es are mentioned with distance from calving front (2692 L 16).

P2692 L20. “High sensitivity and short response time (days) of glacier dynamics to melt periods clearly suggest surface-melt triggered acceleration.” Belong in the discussion.

P2694 5 Discussion This section needs a more thorough discussion and comparison with previous findings on Svalbard and elsewhere (cf. Solheim, 1991; Murray 2000; 2003; Sund 2009; 2014; Burgess 2012; Tangborn, 2013). I suggest first discussing the elements caused by surge dynamics. Then explain how additional factors and mechanisms such as CHW can be found, extracted and separated from the surge dynamics, and finally how these constitutes a possible hydro-thermodynamic feedback.

P2694 L6. How long time is considered to be within “prior to”? It is referred to Fig. 4b. This only shows data from April 2012.

P2694 L18. The last part of this paragraph belongs in the discussion section. While showing the large increases due to surge during a short period, it would also be appropriate to mention the possible influence during the long quiescent phase.

P2694 L25. Dowdeswell et al., 1999 attribute the increase in flow to be a surge or minisurge and the following three phases outlined appears to have similarities with those outlined by Sund et al., 2009. This should be considered in the discussion.

P2695 L7. Parts here resemble other studies on Svalbard surges.

P2695 L8. Please be more specific about what you mean by “the current understanding”, to make it easier for the reader to follow.

P2695 L10. Fig 8 does not exist.

P2695 L1.9 “Ground-penetrating radar (GPR) surveys reveal first occurrence of surface crevasses from 2004 onwards (Appendix C; Fig. 7).” This belongs in the results section.

2695 L24. This stake is not mentioned before, should be in the result section.

2696 L4. Maybe add a reference for “sticky spots”?

2696 L4. Please cf. Murray et al., 2000 to cover further aspects.

2696 L19. Other references on surge termination could be preferentially be added.

2696 L24. Add “in Svalbard” after “drainage basins”. There are surge-type glaciers in other areas that are temperate.

2696 L21 and onward. The text jumps forth and back between surge-type glaciers and ice sheets with no observed surge history. If the authors believe the situations can be compared, they need to explain why the surge context can be ignored.

P2697 L20. Please consider another phrasing than “enormous”, or simply skip. This sentence doesn’t really bring any new information.

References

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